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My intellectual history in relation to my contributions to science

Looking back on my life and trying to reconstruct the main influences that set my character as a scientist and channeled my investigations and thoughts, it seems to me that the main influences were those that stimulated in me and nurtured in me simple curiosity and that, aside from accidental contacts--which were of decisive importance--my investigations and thoughts were directed largely by the pleasure of being in a minimally competitive area and of challenging and opposing currently accepted dogmas.

Early in my life, curiosity took diverse forms. As I recall it, and I recall it vividly, suddenly at the age of 11, curiosity about the cosmos (of which I knew very little) became intense and it soon spread to encompass the whole of Nature, of Man, and of God. Before that, my curiosity was focussed on myself and what directly impinged on me. I have sharp memories of these very personal forms of curiosity. Well before starting to school, I was exploratory about those of my contemporaries who obviously differed strikingly from me,--the Negro boy who was the son of my nurse and who sometimes, not often, played with me; and neighbor girls; inarticulate and vague surprise and wonder about their differences from me. Somewhat later, it must have been after I learned to read, in

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fact it must have been 1911 when I was 5, I was fascinated by the immense volumes, in handsome Morocco binding, of the 11th edition of the Encyclopedia Brittanica. My almost totally blind uncle, Jacob Bamberger, had bought them (and many years later gave them to me). I was in his home the day they arrived and can see myself on the floor with a volume in front of me on the floor--they were too big for me to examine any other way--and looking without comprehension, but with an almost religious awe, at these embodiments of all knowledge, as I was told ~~them~~^N that they were.

To what extent my efforts at school were initially due to curiosity--if indeed they were at all-- I cannot recollect. But I do recollect that a driving force was my father's insistence that my record as reflected in "Report Cards" should show improvement, each report had to be better than the preceding one or my father would not sign the report. I'm sure the consistent improvement he tried to inculcate in me was not always achieved because I still remember my mother saying "He's doing well enough", and always being willing to sign, improvement or not. She was very conscious first of all of my health and thought greater effort on my part might be prejudicial to my health for I was by no means a vigorous child and had more than my share of illnesses, beginning with whooping cough at about

8 or 9 months and continuing with severe colds and influenzas and tonsillitis and so on for many years, including "flu" of the 1918 epidemic (on my 13th birthday) and thereafter heart irregularities.

In the early years, religion made little contact with me except for the concept of God which puzzled me greatly. When still a pre-school child I had a memorable conversation about God with my grandfather whom I recall absolutely nothing else about at that stage of my life or indeed for many years thereafter. His statement that God was everywhere, in answer to my question "Where is God?", left me in an uncomprehensible haze. Yet when I went upstairs to bed, I "saw" God come through the wall, pass through the room, and exit through the opposite wall. This neither surprised nor frightened, nor puzzled me. I took it just as a private experience and never said a word about it to anyone. Although it was deeply imprinted on my memory, it never happened again and I suppose I gave little or no further thought to it.

In spite of my mother's compassion for my failure to perform to my father's satisfaction, she contributed to the nurture of my curiosity. When I was ill in bed, she would read to me; when I was too ill to remain in school,

she tutored me so that I could pass with my class to the next grade. I remember only one of the books she read to me, but that I recall intensely. It was a history of the United States in words of one syllable (as much as possible). And what was imprinted in my mind was the last sentence of the book: "Who knows but that some day you may be President of the United States". I suspect that was the seed that grew into the acceptance of myself as having a future, something to make my life count.

For some years, I failed to realize that other people had an inner life comparable to mine. My self-consciousness was something I simply was unable to generalize so as to recognize that others also had it. I felt absolutely unique and different in kind from everyone else. I was the center and the focus of the universe. So far as I can recall, the possibility of my ultimate extinction seemed impossible or else it never occurred to me. My sense of centrality and uniqueness did not express itself in any way. It was simply part of my secret inner life, my business and no one else's.

Probably because of my mother's reading to me, I needed no further stimulus to lead me to reading. At first, boys's books of the Horatio Alger and Motor Boys type; later, history, biography, philosophy, religion. By 12 or 13, I was reading avidly "good literature", trying to understand

people, observing them, writing about them, trying to make poetry, literary criticism, philosophy; but very little science and that mainly very elementary astronomy and field guides to natural history. I was never a very practical lad and was poor at making things or repairing them,--that was the domain of excellence of my father and older brother. My domain was purely intellectual.

I can't account for that very well. Part probably was due to my father who had an exalted idea of education and who was a tinkerer and "experimenter". He did not go to High School,--had to work to help the family income when he finished 8th grade. He performed simple chemical exercises in a barn when a lad and had the usual accidents and explosions. He knew no algebra, but could solve algebraic problems--simple ones--arithmetically. He loved to "experiment" as he called his probing trials of various practical procedures,--such as trying to make champagne. He easily implanted in me the desire to obtain the education that had been denied him. Remarkably, considering his background and station in life, education meant to him learning and understanding, not training to make a living. From him I imbibed that point of view as natural and unquestionable.

On my mother's side of the family, there was an even stronger tradition of scholarship, though mainly limited to religious scholarship--knowledge of the Old Testament, the Hebrew Commentaries on it, and the corresponding theology and ethic. She herself, however, was in no sense a scholar-- that was not a domain for women-- but a bastion of unquestioning faith.

Probably another contribution to my unreasoned drift into an intellectual life and lack of proficiency or interest in practical matters was my physique and health, weak and sickly. The mind was a retreat from domains in which I could not hold my own. And the pleasures of the mind reinforced delight in the retreat. I lived in books and thoughts, not in action and minimally in play,--except mental play such as checkers and chess in which I became sufficiently proficient to "make" the High School "team". Eventually, however, I did play ball and tennis and swim.

Among my precollege teachers, only two stand out as inspiring me in any way. One was the Principal of Elementary School #61 in Baltimore, Mr. Bothe, a one-armed, mustached, high-pitched soft-voiced, fearsome man who singled me out when I was in the 8th grade and on occasions took me to his office and read to me. Of the books he read to me, I recall only

Sartor Resartus of Carlyle, But he surely encouraged the interest in literature he perceived in me. The other was Injun Joe Reissler, the teacher of English at the Baltimore Polytechnic Institute (which I attended for two years in emulation of my brother who was heading for Engineering). Injun Joe, also perceiving my interest in literature, took me to his home and gave me books. These two men thus reinforced what was already a strong, even insatiable impulse to learn all that literature could teach.

I did not begin to appreciate my father fully until I was 15. At that period of my life and shortly before it, my interest in cosmology, philosophy and religion had led me to begin to prepare myself for the Ministry. I felt the absolute need for having purpose in life and in the universe. Religion answered that need and I wished to devote my life to it. But within a year, I began to revolt against prayers of supplication as inconsistent with my conception of an omniscient and just God. This led on and on to philosophical difficulties and the current cosmology of an ultimately cooling sun and extinction of life on earth left destitute my need for purpose, since I also could not maintain my faith in immortality. The result was

a spiritual, emotional, and intellectual upheaval with intense resentment that life without answers to fundamental questions had been thrust upon me. In a short time I went from absolute faith to questioning to agnosticism and finally to atheism. And for three or four years my inability to adapt to this frustration and collapse sank me in emotional depression. The only bright side of this traumatic experience was that it permitted my father to confess to me that he had long lost faith and had early become an agnostic. He had promised my mother not to let their children know this and he kept his promise until she released him when I went even further to atheism, a transition that gave my mother great distress for she was a woman of unalterable deep faith who believed (as I could not) that there were some things God did not want us to know. From this time on, my father and I grew closer and closer and I became more and more the fulfillment of his thwarted intellectual ambitions.

That is all I can reconstruct of the influences that created and nurtured curiosity in me. During my adolescence, this curiosity was broadly comprehensive about intellectual matters, but--aside from introspection and observations of people--my curiosity sought its satisfactions mainly in

books, and, much less, in natural history. What proved to be the main channels of my investigations and the thoughts were not at all evident until I went to college at Johns Hopkins. Science, as it was taught in High School, did not excite me at all and from my reading I did not realize that science was a continuing process. It was all there in books, finished, and to be learned. My interests were in philosophy and literature and I was intent on majoring in English Literature at College. A sudden and decisive change in me came from a teacher, Ethan Allen Andrews, whose Introductory Biology course was the first and foremost "accidental encounter" that was decisive in my life. He taught me the one thing that, more than anything else, dominated the rest of my life: that it was possible to discover things by observation and experiment; that there is more to learn than is to be found in books. How I had failed to realize and appreciate this up until that June (1922-23) I do not know and cannot explain. I must have been at least dimly aware of it, but, if so, either I was not impressed by it or I thought discovery was possible only by rare geniuses. The lab manual, written by Andrews, contained minimal directions; it was mainly a book of questions to be attacked by observation and experiment. Andrews

didn't expect correct solutions or answers from first hand studies made in the short interval of a three-hour lab. But he did expect careful observation and honesty. We were not told of references to books and were expected not to seek them in the library. Nor did we learn what experimentation properly controlled consists of. Andrews was a naturalist and only a crude experimenter. Yet he excited and inspired me, and he appreciated me. He was nearly at retirement age, but he would sit on my lab desk and talk with me as if I were a contemporary. I plied him with questions, for he had aroused my curiosity to a pitch of intensity I had never before experienced. The results of my lab studies were indeed honest, but usually very imperfect and often quite wrong; but he never corrected the,--only encouraged more investigation--and rewarded my honest observations with the highest grades in spite of their imperfections. Years later, when I was a faculty member in that Department, I learned from other faculty members that Andrews used to show off to them the work I did in his class as if it were marvellous to behold,--in spite of its imperfections.

Although that class was the most important in my education, there were others that had deep impact on me: George Bred's course on the history of

philosophy, the Physics course of Ames and Pfundt; and Munneghan's Calculus. And the great experience of hearing visiting lectures such as Niels Bohr, and Russel. Boas was surely the most succinct and brilliant lecturer I had in any course; and I felt so secure in it that I didn't feel the need to study for the final exam. Although Munneghan thought I was the star pupil in his class, I never felt that I really grasped the Calculus.

Then, when graduation time came and I had to decide what field I would enter as a graduate student, I was at a loss to choose between Physics, Philosophy and Biology. When I consulted Boas, he said he thought I'd do as well as anyone in Philosophy, but that I'd be foolish to go into Philosophy if I had even a nearly equal interest in a science. For trivial reasons, I turned away from Physics though offered a summer job in it; and settled on Biology without any real comprehension of what that involved.

At that time, there were four graduate professors in Biology; but they were in three different departments: Botany (Duncan Johnson, with whom I did take a graduate course and was the only student in it); Plant Physiology (Livingston); and Zoology (Mast and Jennings). I settled on Zoology because

of my interest in Man. Jennings was Head of the Department (which included two undergraduate or so-called Collegiate Professors--Andrews and Cowles, whom I assisted in Comparative Anatomy to get a small stipend and free tuition). Mast had all the graduate students; Jennings had none. Both Mast and Jennings worked mainly on Protozoa, the former on physiological topics, the latter on genetic problems. Actually, during that period (1925-28), Jennings was working mainly on Rotifer biology,--life span, fecundity, and their inheritance during parthenogenesis.

Jennings' courses (one semester each year) were a cycle of three topics: Development, General Genetics, and Genetics of the Protozoa. He was also at that time much in demand as a speaker on the bearing of biology and genetics on all sorts of things. He was one of the foremost interpreters and popularizers of these subjects and of the philosophical implications of biology. His courses were models of thorough scholarly portrayal of the status of the subject, from the viewpoints of critical radical experimental analysis and of general import. His thorough scholarship, objectivity, humanity, and mental keenness attracted me greatly; and his reputation awed me.

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I decided to ask him to accept me as a graduate student; but my awe of him was so great that several times my hand went limp when I went to knock on his door to pop the question. He graciously accepted me and that became the second "accidental contact" to be decisive in the course of my investigations and thought. I became a Jennings protege with all that implies as to his influence on me.

In the old Hopkins tradition of "Here's an organism; go write a Thesis on it", Jennings gave me a finger-bowl containing a creature he had recently collected. He said: Learn how to cultivate it; study its life history; consider what basic problems of biology it would be well suited for investigating; and report back to me and we'll settle on one. He told me the creature was an Oligochaete, Aelosoma, and sent me on my way. That was 1926.

I did not see Jennings again for consultation until I had done what he directed. I found that the creature would eat, grow and reproduce if I fed it a Ciliate which I later found out (thanks to Edouard Chatton who visited Jennings some years later) was Colpidium campylum. My innocence of taxonomic Zoology was so abysmal that it was some years before I discovered that Jennings had given me the wrong finger-bowl and that the organism in it was not Aelosoma,

not even an Annelid, but a flatworm, *Stenostomum*; and, to make matters worse, a new species which I had to christen and describe (as *S. incaudatum*); and, still worse, that I knew nothing about the rules of nomenclature or the necessity to preserve a type specimen. After, in due course, I published my results on this creature, the U. S. expert on the group, Prof. Kepner, of the University of Va. challenged me about it. He said he had never seen the species and asked to see the type specimen. By that time I was working on Protozoa and no longer had live specimens. I likewise had no preserved specimens and wouldn't have had the least idea of how to preserve one if I had wanted to. Kepner was furious at my lack of a type specimen and gave me a hard time. Years later, I'm glad to say, he found the species and sent me a nice letter recanting all his earlier statements that there was no such species.

I can still recall my delight in discovering--or rather just seeing--the most elementary things about *Stenostomum*: the way it sprung open its mouth and pharynx in capturing a *Colpidium*, the way it grew and developed new heads along its trunk; and the active pulling free of a new zooid as it separated from the parent and began a new independent life. Every smallest

detail in learning the elementary biology of the creature simply thrilled me. It was my first and one of the most satisfying voyages of discovery in my life of discovery.

Still true to the training I had in Andrews's course, I shunned the library and studied *Stenostomum* as if no one in the world had ever seen it before. (Besides, had I gone to the library, I'd have read about *Aeolosoma*, not *Stenostomum*!). Quickly, I observed differences between the parent and the offspring, especially in the darkness of the parental head and the lightness of the offspring head. So I could keep reisolating the same parent and follow its "life history". And a life history it had indeed. The parent multiplied rapidly at first, then later developed structural abnormalities and eventually died. But a line of descent of successive offspring from young parents could be cultured apparently indefinitely without decrease of vigor or development of structural abnormalities. So here was a good problem, not unlike what Jennings had been doing with uniparental reproduction in rotifers: the effect of parental age on the progeny. And I could also use this material to study the effects on offspring of harmful conditions to which the parents were subjected. So, I was ready to report to Jennings and did.

He seemed to be pleased because he was at this time intent on extending to developmental and genetic problems the approach he had many years earlier applied to the study of behavior, i.e. to get first the general situation in single cells and then extend the study to lower invertebrates. He had worked on genetics in Protozoa from 1908 to about 1916. Then came the interruption of the 1st World War. When he got back to research after the War, he turned to Rotifer genetics and wanted his students to continue the extension of materials. Emily Emmert was to work on Gammarus; Helen Mar Miller (Costello) on rotifers; and I was to work on Acolosoma (Stenostoman!). So, he said, that I had selected good problems and should go ahead with them. He suggested using lead as a toxic agent for studying the effects on progeny because lead poisoning was then of some notoriety.

So I had my problem and went back to work with little or no further direction from Jennings. It took me little more than a year of further work to have enough results for a Thesis and an additional year to analyze the data and write the thesis. My data were in fact of considerable interest. There was a closed life cycle ending in death of the parent which I found,

when at last I had to read the literature, was quite at variance with previous ideas. It had formerly been held that the reproduction (asexually or uniparentally) in *Stenostomum* (and *Acolosoma*!) was fission, the two products being physiologically equivalent. I showed that this was not true, the "parent" aging and dying while the "offspring" initiated a new life cycle. Further, I showed that the offspring of old abnormal parents were themselves often abnormal and could not initiate a new similar life cycle, but were doomed to early death.

Most interesting and most important for my thinking and future work was my discovery that exposure to lead acetate induced abnormalities which were also transmissible to offspring and, in certain special cases, gave rise to new hereditary types,--2 kinds of doublets. This led me to recognize that rearrangements and changes in number of self-reproducing parts could lead to hereditary differences. This finding was published in 1930 before--long before--geneticists were prepared to appreciate it. Indeed they still are not. It's theoretical importance is in its demonstration that hereditary differences exist which are not due to genic differences. But since *Stenostomum* could not be cross-tied, the full demonstration of this basic discovery carried insufficient force. As will appear, I have later returned to this theme

with full controls and genetic analysis in Paramecium. It is still not fully appreciated. But it bolstered my critical objections to the still current obsession with the gene.

Continuing with my intellectual history and not having here what I wrote yesterday, I may be backing up a bit. I know I was discussing my Thesis research when I broke off yesterday. So I'll pick up there, stressing two features of it---actually three---that were significant in my intellectual history.

First, the Andrewsian imprint of looking before reading may have been responsible for my discovery that there is a life cycle difference between the anterior and posterior product of reproduction. The literature said there was no difference. Had I read the literature, would I have perceived or followed up the perception of the immediate difference in the shade of the two heads? Had the idea of limited viability of non-renewable parts yet appeared in the literature? (To this I returned in a paper on aging now in press; and suggested it might be the basis for the aging difference between Tetrahymena--which can and does replace the oral apparatus when cell size or amputation stimulates it--and Paramecium--which cannot do this during vegetative reproduction, but does so at autogamy

and conjugation; and Tetr. clones can live "forever" while Param. clones die after 300 ± 50 cell generations.)

Second, the development of two forms of hereditary double animals led me to recognize the existence of autoreproduction at levels above the gene and chromosome. This has proved to be one of the leit motifs of my investigative and speculative career. Whether it was an influence which made me a life-long critic of what has seemed to me a blind and erroneous faith in the gene as the source of all heredity, I do not know. Nor do I know what effect a comment of Andrews, when I was a student in his beginning biology course, had. We were chatting about genetics and the gene theory (in 1925 or 26, about a decade after its establishment by Sturtevant's map work). Andrews said "The gene theory is just the fashionable thing now. It will pass, like all fashions in biology." The fact that I've never forgotten that comment may mean that it had more impact on me than I ever suspected until this minute. I've recalled that comment in recent years and cited it in conversations as a sort of joke,--65 years of persistence makes quite a stand for a transient fashion or fad!

Third, the work with lead acetate, the presumed effect it had in yielding —

very indirectly--hereditary variations, alerted me throughout my career to the possibility of hereditary effects of external conditions. This has been another leit motif of my investigative and speculative career. Right up to the present in my analysis of the trichocyst system in stock dl13. It has also--and did even in my graduate student years--made me have closer than distant relations with Lamarckism, with the inheritance of acquired characteristics. At my final examination for the Ph.D. degree, Jennings seized the opportunity to alert me to at least one of the dangers of such an association. He asked me who in the 20th century had claimed to obtain positive results on this, to describe and criticize their work, and to tell what became of them. The latter was the payoff: all had come to a bad end,--Tower had gone crazy, Kammerer committed suicide, etc., etc. At the end of my account, Jennings said "Let that be a lesson to you" or something to that effect. That was a sobering thought; but it had the effect only of putting me on guard, not diverting my attention from the possibility. With the rise of Lysenko in the USSR, this area of research passed from the domain of science into that of politics. And I was drawn into the politico-scientific debate, publishing attacks on the politicization of science in the

USSR. In response, an article (I think it was in Pravda) stated that Sonneborn's research supported the Lysenko point of view, but that he didn't dare acknowledge this in a capitalistic controlled country! Indeed, even in the USA, some biologists confronted me from time to time with the assertion that my researches did in fact support Lysenkoism and they challenged me to explain that away. Until now, it has never occurred to me that I might indeed have been influenced in my attitudes toward my discoveries, in how I interpreted them, by the socio-political climate.

Here I shall try to reconstruct how my experiences and thought set my course in the socio-politico-scientific controversy. My first important experience with Marxism and Marxists came shortly after my marriage when I was a very junior member of the faculty at Johns Hopkins, i.e. in the early 1930's. One of my good friends was Albert Blumberg, a contemporary who was an Instructor in Philosophy at JHU. Ruth and I had run across him in Paris on our honeymoon in the summer of 1929. It was natural for two friends from Baltimore, meeting accidentally in Paris, to arrange to socialize together. Albert was already a Marxist, which did not shock me since many young intellectuals were at that time. So, Ruth and I were interested, but

not seduced, by the group to which Albert introduced us, largely a group of Viennese Marxists. I recall particularly one evening at the apartment of a Viennese woman who we supposed to be Albert's current mistress. There was much Marxist talk,--all (as I recall) on a purely intellectual plane. Ruth and I found the discussions totally unconvincing.

Back in Baltimore, Albert invited me to join an informal discussion group which met at the University. It was a strange group as I quickly discovered. The object was to train for effective oral discussion, to be able to put down opponents, to give the impression of authority, regardless of the facts, truth, or logic. This was not at all to my taste and I withdrew after attending one or at most two meetings, as soon as I realized what the objectives were.

At that time, Ruth and I used to have open house one night a week. All of our friends knew this and came as and when they wished. Conversation was lively and ranged over all things of interest. As a friend, Albert learned of this and asked if he could come. Of course he could, He brought a point of view none of our other friends had and that added to the liveliness of the discussion. It was during these discussions with him that it first

became clear to me that a cardinal principle of Marxist tactics was that any thing--lies, misrepresentation, or worse--was approved if the end or object was "good". That alone was quite enough for me. Marxists could not be trusted and Marxism was evil at the core. From that time on, I was a strongly convinced anti-Marxist and never suffered the fate of the many intellectuals who were attracted to it.

Soon after Albert began to come to our soirées, he started to bring first one, then another of his fellow Marxists, and before long groups of them. In effect, he was in process of trying to convert our beloved soirées into an active Communist cell. We couldn't find it in our hearts to tell a friend not to come to our home or not to bring his friends, so we simply discontinued the whole thing,--we ceased to be "at home" for these evenings. Albert became the head of the Communist Party in Maryland and served a jail sentence.

A few years later, a couple came to our home to inquire about renting it for a summer (when we went to the Marine Biological Laboratory at Woods Hole, Mass.). The man was a writer, his wife a sculptress (who later made a head of Ruth). We were charmed by the couple and they became our good friends. They were Esther and Wittaker Chambers! Witt was amazingly astute

about politics. He predicted--always correctly--European events long before their possibility occurred to us or any other friends of ours.

One evening I drove over to Witt's house and to my amazement found him sitting behind a desk facing his front door with a revolver on the desk within reach. Astonished, I asked what the idea was. He replied in extenso, telling me much about his personal history. Early in his life he had been a revolutionary in Ireland. Later, he became a Communist and rose to high position in the U.S. Communist Party, But, being a Trotskyite, he eventually was driven out of the Party and, because he knew too much, he feared for his life. He said he was being watched continually and that anyone who came to see him would likewise be followed and watched.

Sure enough, next day I found that my locked car (which I parked overnight on the Hopkins campus) had been jimmed and searched! As is well known, a few years later--after we had moved to Bloomington and had lost touch with the Chambers (he had become a leading member of the editorial board of Time-Life, which he told me--he did to protect himself from abduction by Communists)--the Chambers-Nixon-Hiss case broke. Two of my friends (Nixon not one of them!) in public contest. Hiss and I had been students

together at Hopkins and good friends. And we, curiously, had bumped into him in Paris on our honeymoon, never dreaming that this brilliant young man would be accused and convicted of passing highly secret documents to the Russians. I never could bring myself to believe that Alger Hiss was a traitor. Well, the point of this discussion is that I had my baptism of Marxism early and from leaders in the Party, with the result that I knew what it really was and was firmly convinced against it. When Lysenko rose to prominence, I was thoroughly set against him on ideological grounds. Whether this colored my interpretation of my research I cannot say. I don't think it did. It seemed to me then, and still does, that what I found about hereditary effects of environmental conditions is comparable in general to the effects of X-rays, ultraviolet, nitrogen mustard and other chemicals on genes and chromosomes and not at all comparable to Lamarckism or Lysenkoism which claims adaptive hereditary responses specific to the applied conditions. Only in the case of antiserum-induced changes of serotype could there be any specifically adaptive relationship. Perhaps that was the case Pravda had in mind. It at least may merit consideration in that connection, though I never could consider even this case to be a Lamarckian phenomenon. Because it did not operate so as to adjust the antigen to withstand the antibody; it

operated by total replacement of one antigen by another one which the antibody could not recognize.

After the conclusion of my Thesis research, I applied for and was granted the most coveted post-doctoral fellowship available in the USA, a Fellowship of the National Research Council. I wanted to stay at JHU on this Fellowship, but the object of the Fellowship was to provide opportunities for working in a new milieu with different approaches to broaden the training of the Fellow. So I wrote to Max Hartmann in Berlin-Dahlem to ask if he would be willing to accept me. He graciously agreed. At that time, Hartmann was one of the foremost investigators of lower organisms and was developing his hypothesis of relative sexuality on the basis of studies of Algae. I wanted to stay at JHU and follow up an interesting clue I had recognized, of which more below. Well, I was permitted to stay at JHU with Jennings and promptly forgot I had ever applied to Hartmann. Many years later (I think it was 1953) I visited Hartmann. He was then in Tübingen. Immediately he reminded me that I had asked to come work with him in 1928! Fortunately, I covered up the fact that I had forgotten about the great man, who had remembered about the then

totally unknown youngster. This was a sort of negative "accident" in my professional life. What course would my professional life have taken had I come under the influence of Hartmann immediately after my PhD?

The clue I pursued on my Fellowship--after doing a little more on *Stenostomum*--concerned the ciliated protozoan I was using to feed *Stenostomum*, *Colpidium campylum*. I observed that the colpidia frequently failed to complete cell division in some of the cultures. On analysis, this turned out to be due to the kind of bacteria it was eating. So I had to make a modest acquaintance with bacteriological methods. I was much aided in this by a good friend I made at Woods Hole, Moyer Fleischer of St. Louis University. I then isolated the species of bacteria in my cultures and found one that induced incomplete cell division and another that gave normal divisions. More interesting, from the incomplete divisions I derived doublet cells that reproduced true to type for variable periods, selection yielding different degrees of stability.

While doing this work, I had a shocking experience. Edouard Chatton, a leading French Protozoologist (who was the mentor of the great French microbiologists Lwoff and Monod--Nobel Prize winners), came to the USA and

visited the equally great Jennings. Jennings brought him to my lab and asked me to tell him about what I was doing, which I did. Whereupon, in indignation, Chatton exploded and asked whether I knew about his paper, published in 1920 or thereabouts. I didn't! He too had found that incomplete divisions could be induced in *Colpidium* by feeding them a certain bacterium. When Chatton left, I went to Jennings reprint collection (which later I bought after his death from his estate), found the paper and found that the pages in it were still uncut! Jennings hadn't bothered to read it either. So, this was one time when the Andrewsian habit of looking and not reading really got me in trouble. I don't know whether this experience changed my attitude towards reading, but I do know that I soon became extremely history-conscious in science and made it my business to try to know everything that had been published on any topic which I was teaching or researching.

In fact, the summer before I taught my first class (on Genetics, taking Jennings place while he spent a year in Japan--I think it was 1933 or 1934), I became completely immersed in the history of genetics and read all summer

about it in the great Woods Hole library. Full of this, I put it all in my course and half way through the course suddenly realized I'd never even get to Mendel if I didn't drop the history and get on with modern genetics. My poor class! But it was great fun. And years later (1943?) at IU I did go back and give a whole course on 19th century theories of heredity, using Yves Delage's great book as a major source. I've never lost my sense of history in science and think I've conveyed it to a number of my better graduate students--Jim Berger more than any other.

Towards the end of the second year of my Fellowship, the question of a job was kept coming up. At the end of the first year, I had applied for a renewal, something I could not at all count on receiving. My personal plans depended on that in more than one way. Ruth and I had secretly planned to marry and go to Europe on our honeymoon if the Fellowship was renewed. That was early in 1929. The US economy was flourishing (apparently); the stock market was in a powerful bullish phase; speculation was high; everyone had visions of richness. Each year of the Fellowship permitted six weeks of vacation and I planned to take the last 6 weeks on the first

year and the first six weeks of the second year (if that came through) to give us a long three-months of honeymoon. So I asked the secretary of the Fellowship Committee to phone me as soon as the decision was made. This she kindly agreed to do and, when I got the good news, Ruth and I proceeded with our plans, reserving places (tourist third class) on the volendam of the Holland-America Line, bound for Rotterdam. I felt confident, that, with the honor of two years on the NRC Fellowship and with Jennings' assurance as to my job prospects (he had told me that he thought I'd have no difficulty getting a good job in spite of the widely prevalent difficulty of Jews in finding suitable academic posts), ~~and~~ my being able to support myself and wife after the Fellowship would present no serious problems. So, off we went to Europe for a memorable marvellous three months,--June through August.

Returning to JHU in September I got back to my research in high spirits. Then came the crash. Within a month or two, the stock market bottom fell out and the Great Depression began. As the year went along, Universities began to feel the pinch and jobs became scarce. Two possibilities emerged: one at Washington University in St. Louis (to teach Comparative Anatomy,

in which I had assisted at JHU for a few years with Professor Cowles, but a subject far from my interests, yet I dreamed of how I could vitalize that dull subject) and one at Yale (to replace Woodruff in Protozoology, much closer to my interests). Neither place called me and Jennings had to admit to me that, under the constricting economic conditions, Jewish candidates would be at a severe disadvantage.

Things looked pretty dire for us, but Ruth still had her social work job and her salary was about the same as mine,--\$2200 a year. Then fortune smiled on me. The Rockefeller Foundation made a grant to Jennings to enable him to return to Protozoan Genetics on which he had worked from 1916 to 1928 or 29. His work on Paramecium (1908-1913) had used mainly statistical methods which, at that time, were so foreign to biologists that they did not appreciate the validity of the important conclusions Jennings had drawn. Or at least that was how he evaluated the situation. Meanwhile, at Columbia, Morgan and his group had developed Mendelian genetics to the heights of the Chromosome and Gene Theories. So Jennings planned to return to the genetics of Paramecium, but use "biological" instead of purely statistical methods to demonstrate that conjugation resulted in yielding

clones of diverse "biotypes". He had already started a graduate student, Daniel Raffel, on this and Raffel--a very bright and forceful man--had attempted to interpret all biotypic diversity as due to mutations. But again the approach was ~~XXXX~~ largely formal, forcing the data into speculative genic interpretations without ever demonstrating the existence of a single gene. When Jennings got the Rockefeller grant, he offered me a post as his Research Assistant, spelling out in detail what my duties would be and including among them the obligation to go to Woods Hole each summer at my own expense to work with him there. Of course I jumped at the offer and started my life with Paramecium in 1930-- in the summer and at Woods Hole.

There Jennings, his long-time secretary-assistant Ruth Lynch, Raffel, and I began our joint efforts. Before the end of the summer, Jennings and Raffel had a knock-down conflict about whose name would come first on resulting publications and, as I recall, also on who would write the papers and be responsible for their contents. Raffel felt that, since he had been doing the work before the rest of us and had arrived at his own methods (simplified and more standardized medium) and conclusions (as to

the mutational basis of the results), his name should come first and he should have the privilege of making the interpretations. Jennings recognized the weakness and formalism of Raffel's interpretations and flatly refused to give in to him. He told me "I cannot work with a man like that". In the end it was decided that Raffel would write his special part under his own name alone and that he would be a junior author of the first group paper and that thereafter the relations of Raffel to the rest of the group would be severed. Actually, Raffel then left JHU and went to Russia to work with Muller on position effects. He got along well with Muller, who often talked to me about him during his years at I.U.. He never could understand why Raffel and Jennings (and Raffel and I) had been unable to work harmoniously.

I was much more naive than Raffel, much more an observer and closely bound to demonstrable results, much less hasty and theoretically ambitious; so I got along well with Jennings, himself a radical experimentalist.

Raffel never got a university position and lived out his life as a teacher in a private school (Park School in Baltimore) and as a cattle breeder.

I stayed on at Hopkins with Jennings and was quite content to be the most

junior author of our first (and only) joint paper (of 1932) on the diverse biotypes arise at conjugation within clones of Paramecium aurelia. From the beginning, my approach to the genetics of Paramecium diverged from that of Jennings, but I made no fuss about it, just went quietly and calmly on my way. As his Research Assistant, I of course felt obliged to do what he wished done and in the way he wished it to be done. In fact, during that first summer with him at Woods Hole (and earlier), I made a point of observing closely how he went about scientific work. I kept a notebook just on that. He was the only famous scientist I knew well and I wanted to find out the basis of his fame, what qualities and approaches and methods he had brought to his work. I learned a great deal, just by keeping my eyes and ears open, not by questioning him.

During that first summer, I came to the conclusion that there were two basic needs to be satisfied in order to obtain insight into the genetics of Paramecium. The first and easily satisfied need was to go beyond mere validation of clonal diversity arising at conjugation and see whether it would be possible to select diversities such that the different clones would breed true through further inbreeding and to select as well for

reduction of variability at successive conjugations. In my free time that summer, I began to work along these lines and continued it during the academic year at Johns Hopkins when Jennings was too busy with teaching, administration, outside lecturing, and writing, to have any time left for research or for the need of a Research Assistant's services. My work led to a paper (I think jointly with Ruth Lynch) in which I did demonstrate the selection of true breeding differences and the total elimination (or almost total) of genetic variability at further conjugations. This was contrary to Jennings's earlier work (1913-16) in which he kept getting hereditary variations at successive inbreedings and with no evidence of decreasing amount of variation. It was this that had led him to mathematical analysis of inbreeding,--one of the first and most fundamental contributions to mathematical or population genetics. It was also what led him to conclude that "Mendelian recombination might not be the whole of the matter". (Indeed, as Jollos's analyses were showing at the same time, it was not the whole of the matter. But Jennings never really accepted Jollos's results as significant because they were erratic and not reproducible,--not

even by Jollos himself. However, as I showed many years later, Jollos's Dauermodifikationen were more significant than even Jollos realized.)

Finally, Jennings imagined that the inbreeding results he obtained held out the hope or prospect that genetics of unicells might reveal more primitive systems than the Mendelian system known to apply to higher organisms. So, my demonstration of the results of selection was probably something of a blow to his ideas. But he took it well because he had utmost respect for observations and experiments and loved Hunter's old maxim "Don't think; try." (Nevertheless, Jennings was a thinker, but only about the results of experimental trials and solid experiences--including introspective experiences.)

The other need, one that no one knew how to satisfy, was to be able to cross-breed the diverse types one obtained, i.e. subject them to Mendelian analysis. This need became my obsession. I saw that it would be difficult, if not impossible, to demonstrate non-Mendelian phenomena unless one could exclude a Mendelian basis by performing a Mendelian analysis. And that could be done only by being able to cross genetically different types.

It was astonishing to me that no one had--so far as I knew--ever done this in spite of the fact that conjugation had been observed for more than a century and that leading investigators of the 19th century (e.g. Richard Hertwig, Emile Maupas, Bütschli and others) and of the 20th century (Enrique, Calkins, Jennings, Chatton, etc.) had worked extensively with conjugation.

Jennings's conclusion about this, based on his 1908-1913 studies, was in direct contradiction to that of Maupas, the only two investigators who seem to have been seriously concerned with the problem. Maupas, working mainly on Ciliates other than Paramecium aurelia, had concluded that "Nature abhors incest" because, repeatedly sampling his aquaria, he found conjugation only when more than one collection from nature was implanted in the same aquarium; or, if only one was there and the strain conjugated, the resulting animals died. (My later work suggests why the death occurred: I found that mutations accumulate with clonal age and that old clones cannot yield viable conjugant progeny.

Jennings was a careful student of Maupas's papers. He tried two kinds of experiments with Paramecium. (1) He started clones from single cells and kept looking for conjugation, ~~XXXXXX~~ following Maupas's generalization

that conjugation occurred only when food gives out after a period of rapid reproduction. He obtained conjugation this way with Paramecium and, unlike Maupas, found that these which had conjugated lived well and grew vigorously. Moreover, he obtained series of successive conjugations in rapid succession, with the same result. In his experience, therefore, Nature did not abhor incest and there was no requirement to have different strains living together (or to conclude, as Maupas did, the viable exconjugants were confined to crosses between different strains). Be it noted that Maupas never really proved that conjugation occurred between different strains; he merely inferred it from the observations that led to his dictum about incest.

The second kind of experiment Jennings performed was an attempt to repeat, with built-in "markers", Maupas's conditions for viable conjugation. He chose visible markers, chiefly differences in size or shape or both. Having found strains in nature that differed in these respects, he grew them together and looked for conjugation between cells differing in size and/or shape. What he found was the opposite of what Maupas had claimed.

When conjugation occurred, the two mates appeared to belong to the same strain for they were alike in size and shape. Moreover, at any one time, usually only one of the two ~~XXXXXX~~ ^{strains} was conjugating; but when two were conjugating, each was conjugating only with those of its own size and shape. So, Jennings concluded: (1) that the conditions for conjugation differed for different strains; (2) that there was "assortive mating such that only like cells conjugate with each other. He therefore abandoned the hope of crossing diverse type, i.e. of making a Mendelian analysis.

This was the discouraging atmosphere that prevailed when I set myself to break through the impasse and learn how to cross-breed P. aurelia. I had already made the first step during that first summer on my new job and during the following academic year in Baltimore. I had two "pure-breeding" strains that had clearly different phenotypes. So I had the material needed for crossing. But I lacked a method.

The perception of a possible method soon came to me. Conjugation occurred within each of the two diverse strains. And of course it had long been known that conjugation began--if it was going to occur at all--when the animals begin to stop dividing due to depletion of the food

supply (bacteria). So my idea was to try to control the populations and their food supply so that both strains would start to conjugate at about the same time; and, if that succeeded, to isolate one cell from each culture into the same minute droplet so that they would have to bump into each other repeatedly. In other words, get both cultures into conjugating condition simultaneously and then make single pair combinations in the hope they'd conjugate. How exciting it was to see that it worked! Not all pair combinations, but sometimes up to 50% of the pairs did. (Later, my discoveries were to clarify this 50% limit, and all the conflicting observations of Jennings and Maupas.) So, I had succeeded for the first time in history in crossing two diverse strains of a unicellular animal. The only success with any unicells prior to this was with Chlamydomonas, and other green flagellates, all considered to be plants at that time. And in no animal simpler than an insect had Mendelian analysis been carried out. So I knew I had something pretty thrilling in my hands,--best of all, a method that should work eventually even if the material at my disposal failed.

Well, in the most important sense, it did fail. For I did not have in

my materials simple, unequivocal, single gene difference. The numbers segregating in the F2 (the F2 by conjugation within single F1 cultures) did not agree with expectations for a single gene difference and, to reconcile them with ~~the~~ gene theory, I had to postulate more than one gene difference and assume certain genic interactions. As a first and model example, this was a first. But it told the biological world that Ciliates could be forced to cross-mate, that Mendelian analysis was going to be possible, and that probably that young upstart Sonneborn would be the one to do it. And it told more,--that there could be a fairly long period of what I then called cytoplasmic lag (and what later came to be known as phenomic lag when others began to work on bacterial genetics). This was a lag in time and/or cell generations before the new genotype established by fertilization came to phenotypic expression and the pre-fertilization phenotype disappeared. Actually, De Garis in Jennings's lab had reported this earlier in what he claimed were species crosses achieved by chemical means. Moreover, both De Garis and I observed that the two mates of a pair came eventually to produce clones that were alike in phenotype. He reported that the final phenotypes were different

for different pairs of the same species cross. I think, as I recall it, that the F1 pairs all came to the same phenotype, but F2 pairs varied, in my work. The explanation of De Garis's results is still obscure. Mine fit expectations if, as should have happened, the two mates of a pair acquire identical diploid genomes.

I was encouraged by my partial success, but not satisfied with the method or the analysis or the material used. So, I set out to discover the conditions required to make cells conjugate. Routine control of conjugation required full knowledge of the conditions that induced it. Moreover, there was that strange process called endomixis reported in 1914 by Woodruff and Erdmann in the very species we were using, P. aurelia. What effect, if any, did it have on hereditary traits? Could it too be controlled or was it finally periodic as Woodruff and Erdmann maintained? And what about Woodruff's claim that some strains of P. aurelia couldn't conjugate at all or did so very rarely? I resolved to clarify all of this mystery by learning the whole general biology of the cytogenetic processes occurring in P. aurelia. This was the program I set myself after the abortive Mendelian analysis of 1933-34 which, though certainly imperfect and not up to my hopes for it, did go some distance towards

bringing the genetics of unicellular animals into sight of the new Neo-Mendelian revolution.

My revised and updated program of research seemed, for the first two years (1935-36) to be moving slowly, but steadily; and then the following year (1937) burst forth in a brilliant flash that initiated a new era in unicell genetics, opening wide the gates to the rapid laying of foundations for all subsequent genetic work with such organisms and for the discovery of a number of theoretically important new phenomena. The work of 1935-36 did not appear in print until 1936 and was not completely published until 1938, though in the meantime (1937) papers on the main break-through were appearing and causing a great stir and putting the '35'-36 work (which I'll write about first) in the shade.

Looking back from the perspective of time, I can now see that the key to the success that was to come, i.e. opening up the possibilities for modern genetic analysis, was my intuitive feeling--never set forth as a logical necessity--that "endomixis" was important for my purposes and that I should first of all learn to control it and to be fully aware of any possible genetic consequences it might have. This retrospective evaluation

is objectively validated by the fact that 5 of the 6 papers published on work of 1935-36 dealt with "endomixis", one of them specifically with the origin at "endomixis" of a genetic variant characterized by apparent inability to conjugate,--the first of several "can't mate (CM)" variants later to be discovered.

Most of my studies on endomixis, however, dealt with its relation to the clonal life cycle. I found a systematic relationship between the time (and number of generations) since the last "endomixis" and the stage at which the next "endomixis" occurred and the relations to growth rate, abnormality and cell death. But by far the most important finding was the relation between the occurrence of one fertilization (conjugation or autogamy) and the occurrence of conjugation (again). Although this proved merely to be due to the fact that older clones go promptly into "endomixis" when they starve, while younger ones do not,-- and so respond to starvation by being capable of conjugating, the method used in this study proved of critical importance. The method was to carry daily reisolation lines descended from a number of different "endomictic" or conjugant cells and to put the "left-over" cells, after

each reisolation had been made, together into one culture.

Using this method, I discovered mating types in Protozoa. It happened one night while Ruth and our two boys (Lee, 5; David, 4 mos. old) were in Philadelphia to help celebrate her father's birthday (February 28, 1937). I had earlier noticed that the cells of a clone hardly ever conjugated when the daily left-over cells (i.e those remaining after one cell had been isolated to start a new subculture) were allowed to multiply until the food was exhausted. Yet when the left-overs from a dozen or so clones were combined into one culture, sooner or later--when the food was exhausted--conjugation regularly occurred. I had a set of such clones going at this time. During the day, while they were still well-fed, some of the left-overs from all clones of the set were mixed together and I kept observing the mixtures to note just when they started to conjugate. It happened between 1 AM and 2 AM. Then I went back to observe the animals that hadn't been removed from the original clonal cultures. They were starving but not conjugating. I wanted to see whether they would conjugate as soon as I mixed samples of them together or whether--even though starving--the different clones would have to be together as long as those that I had mixed earlier in

the day. So I started putting samples of the different starving clones together, watching the mixtures carefully as each new sample was added.

What I actually saw was neither of the two alternatives I had foreseen as possibilities. After the first few (3 or 4) clone samples had been brought together, the cells began immediately to form, not conjugating pairs, but agglutinated groups of many cells. At first these groups were only 2 or 3 cells but they quickly built up into enormous groups, a large fraction of the cells in the mixture being in one or another of several large groups. This was the first time anyone had ever seen the initial agglutinative mating reaction in any unicellular animal. My excitement mounted by the second. I now went back to these several clones and chose one to mix separately with each of the others. Let's call this one clone A and give letters--B,C,D,E,F,--to five other clones. So, my mixes were A with B, A with C, A with D, A with E, and A with F. Immediately I saw the same agglutinative reaction in some of the mixture, e.g. A with C, A with E and A with F; but the reaction didn't occur in A + B or A + D. Why? Were B and D not reactive? Or--could it be that B and D were the same "sex" as A, while C, E, and F were another "sex"? This was easy to

answer: mix B with C and with E and with F; and do the same with D. To my absolutely ecstatic delight, all of these combinations of two clones gave the immediate agglutinative reaction. I now had two "sexes": A, B and D were one sex; C, E, and F another "sex". That night, and for some time after, I thought of them as sexes. I then took one of each type and mixed samples of all of the beginning starving clones I had with each of my two "standard sexes" (I called them sex I and sex II); and every "unknown" clone gave the reaction with one of the standards--some with A=I and some with C=II and not with the other standard. So, every clone could be assigned to sex I if it reacted with sex II; and to sex II if it reacted with sex I. And all of the clones proved to be either sex I or sex II. Only one thing remained to be seen: did the reaction, the group formation or agglutination, have anything to do with conjugation. By this time a couple of hours had passed and when I looked back at the mixtures that had begun to agglutinate two hours earlier, I found that the groups had disappeared and instead there were many pairs of tight united conjugatns. Turning then to the mixtures that had been made more and more recently, I saw the whole picture: at first the building up of large groups

in a matter of minutes; persistence of the groups for about 1.5 hours; then gradual break up of the groups into pairs united only at the anterior end; and finally to fully joined conjugant pairs. So it was all proved: there were two "sexes", I and II, in this stock (one that I later called stock S for Sonneborn and that had been collected at Cold Spring Harbor, N.Y., I think by Wichterman who sent it to me); when "ripe", i.e. beginning to starve, and not too old, mixtures of the two sexes immediately agglutinate and later pair off in conjugation. It was now very late, perhaps about 3 AM.

Tired? That I don't recollect. Excited? Decidedly. I knew I had opened up the whole field of genetics of unicellular animals. And I had to tell someone,--show them the marvellous immediate mating reaction. My lab was in Gilman Hall, Room 116. I roamed the corridors looking for someone to nab, to talk to, to demonstrate the reaction. No one to be found. Eventually I found on an upper floor an old shuffling Negro janitor. I told him he just had to come to my lab, which he obligingly did. I said, "Look at this!" and, sitting him at the microscope, made a mix of the two sexes for him to see the reaction. He quinted and squirmed and tried hard

to see,--I doubt if he saw a thing. But, hopefully, I said: "Do you see it? You're the second person in the world to see that! And no one ever saw it until tonight". He gave up and looked at me with a deep, warm sympathy, saying "Boss, it sure must be wonderful, cause you're mighty excited". I could have hugged him. Now I could go home, try to get a few hours of sleep, and hurry back next morning to demonstrate and explain it all to Jennings and to any one else who'd let me.

Of course Jennings appreciated immediately the full importance of what I had discovered. Years later, after his death, I read in his diary at the American Philosophical Society Library that my discovery would earn great credit for the JHU laboratory of zoology. He set up a special evening demonstration that I was to give to all the faculty and graduate students and most extraordinary for him, he brought his wife to see it.

He was of course a great enough person to show no trace of jealousy and to be delighted at the boost this discovery would give to the field in which he had been the recognized world leader. He was then 69 and would have to retire a year later. A few years earlier, he had confided to me that he felt the need to do some good research so that biologists wouldn't

think he was "on the shelf". It was this feeling, I suspect, that lay behind two events of the preceding few years.

It was in 1933, I think, that Jennings told me Paramecium simply wasn't in condition for genetic work and, since he had only six years till retirement, he was going to leave it and go back to work on *Diffflugia*. In 1916, when he last worked on it, he had done some preliminary operations on the "mouth" of the shell and observed the effect on the mouth of the offspring. He now went back to this with great success and published in 1936 (or 37?) a very important paper the full significance of which did not become evident until 20 to 30 years later when the replication of DNA by formation of a complement on a one-stranded template led to the idea that this was the basis of heredity and uniquely a property of nucleic acid. But Jennings had shown experimentally that exactly the same mechanism occurs on the supramolecular level: the reproduction of the mouth of the shell involves the forming a negative replica of the ring of alternating teeth and spaces, a tooth forming in the new shell along the space in the old shell, and a space in the new along a tooth in the old.

At the time Jennings announced to me his abandonment of Paramecium, I was

still technically his Research Assistant; but I asked him if he would permit me to continue with Paramecium and let me try to put it into condition for genetic analysis. He generously agreed to this. As it turned out, it was a wise decision for him personally because soon after I found mating types ("sexes"), in P. aurelia, I also found them in P. bursaria and discovered (as I had meantime for "syngens" 2 and 3 of P. aurelia) that there was a daily cycle of a period of mating reactivity alternating with a period of inability to react. I offered to turn the P. bursaria mating types over to Jennings and he promptly accepted and spent the rest of his life working on that species most productively. So, now that Paramecium was indeed in condition for genetic analysis (which Diffflugia was not), he came back to it.

The second event was the only one in my long association with Jennings in which I felt he was unjust to me. In 1936 there was to be an International Congress of Zoology in Madrid. Jennings was to present a paper on unpublished research. He had none of his own to report. He decided to report my work on the effect of autogamy on rejuvenescence. Of course I supposed he would report it as my work, but he reported it as joint work with him as first

author. I learned from this experience how Raffel must have felt and I never understood (and never asked) how he justified and rationalized this to himself. He did absolutely nothing of this research,--neither suggested the problem, nor directed the work, nor arrived at any conclusion I had not already arrived at. He merely took it all over, prepared a speech about it and let it be published. After that I felt no desire to republish all that work in full under my own name.

I should here report another event that tells something about me. The Provost (or Dean?) at JHU was a palaeontologist named Berry. He was a "diamond in the rough"; nothing smooth or polished about him. Although he lacked a Ph.D. (and maybe a B.A.) he was one of the leaders in his field. One day, about a year before I discovered mating types, he came to my lab to talk to me. At that time, I was no longer Assistant to Jennings but was a young faculty member. Berry talked kindly to me, surely with the best of intentions. The burden of his remarks was something like this. "Sonneborn, I hear you are a bright and promising young man. For the good of your future, give up your tenacious adherence to work on Paramecium. Jennings has discovered all that's worth discovering about

that organism. Shift to something else; work in an area where there is much of importance to be discovered." This startled me, but didn't discourage me, and a year later I discovered mating types.

I should also come back again to Jennings's reactions to that discovery. It contradicted his contention that like mates with like (assortative mating) and succeeded where he had failed in bringing under complete control. Did this hurt his feelings? Wouldn't he have been less than human if it had not. I know what it is like--as I shall tell later--for something very much like this occurred to me later and, in another connection, I went to great lengths to soften the blow when I made discoveries that showed those of the famous Yale biologist, Woodruff--a fine gentleman--to be wrong. But that is going ahead of my story.

Well, news of the discovery of mating types spread fast and far. The Baltimore Sun paper, one of the very good dailies of that time boasting H.L. Menckon among other luminaries on its staff, ran a full page feature on me and my surprising discovery (surprising of course because Paramecium is hermaphroditic and whoever would believe there could be sex differences between hermaphrodites?). My family physician, Dr. Nathan Herman

of the JHU Medical School, a salty, straightforward guy with utter disdain of bedside manner, told me in no uncertain words "You'd might as well quit research now. No one makes two discoveries as important as that. You're over the peak (at 31!) , so relax and enjoy life."

At Woods Hole during the summer of 1937, it was arranged for me to give a public demonstration of the mating reaction in P. aurelia syngen! That was a memorable day. The great ones of my predecessor ~~JENNINGS~~ generation lined up in my lab waiting their turn to see the new finding. Among them I remember (still with some feeling of exaltation) the tall, slender bearded face of T. H. Morgan--leader of the school that established the Chromosome and Gene Theories and first geneticist, or non-medical man for that matter, to win the Nobel Prize in Medicine and Physiology. He came as humbly as anyone to see this new phenomenon and, after he had seen, asked me questions, e.g. How can you reconcile this conjugation of unlikes with Jennings demonstration of assortative mating (conjugation of like with like)? How in 100 years of research on Paramecium by such first class investigators as Butschli, Hertwig, Maupas, Calkins, Woodruff and especially Jennings, could so striking a phenomenon as this have been overlooked or

fail to be discovered? But none reacted quite like the professional Protozoologists--Calkins, Wenrich, and others. They were among the ones to which Morgan's question applied. I suspect they felt a little chagrined. But I recall only Wenrich's comment : "Now, I've seen it and I still don't believe it."

I didn't spend all day of my time demonstrating and exulting. I worked like mad following up the possibilities opened up by my discovery. First of all, I studied the inheritance of mating types in stock S (later christened stock 19). (In my first paper in PNAS, July 1937, the two types were called Sex I and Sex II, but within a year--by the time my papers of 1938 appeared--they were called mating types I and II.) The 1937 PNAS paper proved to be the fundamental basic paper of a new era in the genetics of Ciliates. I reported a way of testing whether the final pairs were consisted of two cells of the same mating type and gave the results: it didn't happen at all. But I also used this method (a single marked cell of one mating type introduced into a culture of the other mating type) to show that cells which emerge unpaired from the large clusters of agglutinants can transiently adhere to cells of the same mating type. I tested and found

inactive the fluids (filtrates and centrifugates) in ~~the~~ which the cells had lived; the reaction required the actual presence of both types of cells. I showed that stock S was unable to react for about a week after conjugation but could react within three days after "endomixis" although reactivity was lost during "endomixis". And that clones remained reactive until they were old enough to go into endomixis. Overfeeding inhibited reactivity, but complete starvation was not required, greatest reactivity occurring when the cells of a culture still contain many food vacuoles.

On the genetics of mating types, I reported no exceptions to the rule of constancy during asexual reproduction; but that exconjugants produced three kinds of clones: some pure for I, some pure for II, and some producing both I and II. In the latter, there were two pure subclones--one for I and one for II--the two segregating at the first cell division after conjugation. With marked conjugants, I showed there was no correlation between the mating type of the parent and of its ~~exconjugant~~ exconjugant progeny. Mating type was redetermined at random after conjugation. Exactly the same rules of inheritance and determination were found after "endomixis". I recognized and laid out the evidences for the

macronucleus being the ~~mixis~~ organ that was determined for mating type, determination occurring only when macronuclei arose anew from a fertilization nucleus. I tried to imagine possible genic and chromosomal mechanisms that underlay these results, but recognized that ~~xxxxx~~ resolution of the problem required "a fuller knowledge of the cytology and genetics of conjugation and endomixis".

Finally, I reported preliminary results on four other stocks; Woodruff's classic "Paramecium Methusaleh" (stock W, later #23), which showed on senescence when allowed to undergo "endomixes" and two others (I think stocks G=7 and E=5) from Maryland did not react with stock S mating types; but the stock R(=#18, which I named for Raffel, and which had been the stock we all worked on from 1930 on) had the same two types as stock S, differing only in the fact that subclones from a first fission product could often conjugate without mixture with anything else. In an important footnote I added that this stock sometimes formed more than two macronuclear anlagen and that in a corresponding proportion mating type segregated at the second instead of the first cell division. And I noted that my evidence for macronuclear determination confirmed the report of Calkins

and Gregory (1913) of genetic differences correlated with different macronuclei within a clone, but did not confirm the Chattons (1931) and Zweibaum (1912) that environmental conditions alone determine conjugation. This shows that I had not yet appreciated or perhaps even been aware of the relevance of Jollos's work of 1913 and 1921 on Dauer-modifikationen. And, with hindsight, I can now see that there was no fundamental conflict with a less extreme form of the conclusions of the Chattons and Zweibaum, as was many years later to be shown by Miyake's brilliant success with chemical induction of conjugation. This is an important lesson which I eventually learned and which many biologists even today have not learned, namely, that proof something happens by one mechanism doesn't exclude the possibility that other mechanisms may bring about the same result. It is a hard lesson to keep in mind in the first ecstasy of discovery.

I ended this first paper on mating types with a boast that proved to be entirely justified: "It may perhaps be said that with the present work the genetics of Paramecium enters the quantitative and predictable stage, with tools and methods of analysis which should lead rapidly into

a systematic, coherent body of knowledge in close touch with the rest of genetic science".

I could as well have said "unicells" instead of Parameicum. What I could not then have said, but what proved to be true in the years ahead, was that the "body of knowledge" which would emerge not only would include some "in close touch with the rest of genetic science", but much that was novel, pioneering the exploration of phenomena that were not at all in touch with the rest of genetic science, and that the major thrust of my future contributions would be to extend the domain of genetics into new channels. About that I shall have much to record here.

After Jennings came back to Paramecium, which was a month or so after my discovery of mating types, his work on P. bursaria and mine on P. aurelia proceeded independently to discover some similarities and some differences. In the P. aurelia work, I was quickly joined by my first graduate student, Dick Kimball, whose problem was to study the inheritance of mating types in stock S at endomixis. I knew, before he began, most of the qualitative results, but he did the quantification of them. He also found that about 3 or 4% of the caryonides were hereditary selfers in which he studied how types shifted and found a relation to the parental mating type which has never been reexamined and needs to be. Either there is something unknown to be discovered or his small scale correlations were simply a random fluke. As I recall it, he claimed that stable types arising in a selfer were always the same type as the prefertilization parent. If so, why? No one knows to this day. Kimball also found great variation in the frequencies of the two mating types after endomixis and here too there was one ratio that has never subsequently been matched,--one in which by far most of the caryonides were type I. How is this to be explained? No one yet knows.

Jennings and I began to collect from nature as many stocks as possible from different natural sources. We both found that what had been considered a single species was really several. At first we simply called them groups, then varieties, then syngens and finally species. The basis for this grouping was the capacity to give the sexual reaction, form conjugant pairs and yield a viable second sexual generation, i.e. the capacity for genes to flow between stocks marked them as belonging to the same variety, syngen or "biological" species. This was the same as saying that the same mating types existed in all the stocks of any one syngen. During the first year, I examined many (2-3 dozen) wild stocks and found that they fell into 3 well-defined syngens designated simply as 1, 2, and 3. There were two mating types in each syngen,--I and II in syngen 1, III and IV in syngen 2, V and VI in syngen 3. All stocks of syngen 1 had some caryonides of type I and some of type II, except several stocks collected from a monastery stream at Woodstock, Md. These monastic stocks produced only type I.

Genetic analysis--the purpose to which all my effects that led to the discovery of mating types were addressed--was now a routine matter. So I crossed the monkish stock (P or 46) to the usual wild type (or two-two)

stock and found that the F1 was wild type, the F2 segregating 3 wild type to 1 monkish (i.e. confined to type I). The characterization of the clones was rendered easy because of another important discovery. The frequency of type II in wild type increased with temperature during a sensitive period, the first cell cycle after fertilization, i.e. the macronuclei became determined at that time. (Nearly 40 years later, I found a similar situation for another character, trichocyst discharge.) This was the first clear demonstration in any organism of hereditary nuclear ~~xxxx~~ differentiations impossible on genically identical nuclei, but its general importance was not widely recognized until 15 years later when Briggs and King obtained evidence for it in Amphibia by nuclear transplantation.

This information about the temperature effect permitted us to carry out the breeding analysis at high temperatures that yielded about 90% type II caryonides. Since we found that the two conjugants of a pair required identical genotypes (proving that the nuclear division giving rise to gamete nuclei was a mitotic division of a haploid nucleus), the probability that all four caryonides from a wild type pair of conjugants would be type I was $(0.1)^4$ or 1 in 10,000, we could safely assume that any pair which had all

4 type I was like the mutant parent (pure for I). That was how we got the 3:1 ratio in the F2: there were 3 pairs in which at least one caryonide was type II to 1 pair in which all four were type I. This was the expectation for a single simple Mendelian difference. Hence, the pure I parent was homozygous for a recessive gene (mt^I), the wild type was homozygous for its dominant allele (mt^{I+}). We had for the first time demonstrated that Mendelian genes and typical genic inheritance occurred in a unicellular animal.

The same analysis was extended to "endomixis". To our astonishment, I found that when "endomixis" was induced in the heterozygous FI, the result was 50% pure mt^+/mt^I and 50% pure mt^{I+}/mt^{I+} . No heterozygotes were obtained. The inferences were clear: (1) endomixis was really self-fertilization, i.e. autogamy, as Diller had claimed from purely cytological observations in 1936; and (2) the self-fertilization occurred by union of two sister reduced nuclei produced by division of the same haploid nucleus, in contradiction of Diller's cytological claim that any two of the 8 nuclei producible at the 3rd nuclear division could unite.

So this first genetic analysis in Paramecium--the aim of all of my work

from 1933 to 1938--yielded several basic discoveries: (1) the two mates of a pair produce genically identical clones; (2) the third nuclear division of conjugation and autogamy is a simple mitosis; (3) gamete nuclei are division products of the same haploid nucleus; (4) endomixis is really autogamy in which only homozygotes arise from heterozygotes, thus making F2 by autogamy the method of choice for genetic analysis; (5) Protozoa have genes that behave in inheritance exactly like genes of higher organisms; (6) sometimes conjugant pairs fail to cross-fertilize, each undergoing autogamy, thus confirming Wichtermann's cytological observation that it can occur, but contradicting his claim that it always occurs.

My correction of the faulty observations and interpretations of Diller and Wichtermann, made by direct cytological observations, demonstrated the greater reliability of genetic analysis than direct observation when it comes to very minute objects. And thereby the basic cytogenetics of P. aurelia was established.

Except for the genetic analysis in syngen 1, mating type genetics at first made little progress, except that I found the same caryonidal system in syngen 3 in which I also did much of the work on temperature effects. But

I was not successful with syngen 2 mating types or genetic analysis of any other trait. It was very frustrating. Nothing seemed to happen. Each member of a conjugant pair produced a clone exactly like its parent, the type III mate producing a type III clone and the type IV parent a type IV clone. Could Wichtermann's claim for universality of cytogamy be applicable to syngen 2? The answer was not to come until syngen 4 was discovered and, with it, a gene difference, after my move to Indiana. Before that I had three stocks-- 29, 32 and 47 which did not mate with syngens 1, 2, or 3 or with each other. The reason was that they were all pure type VII, as I was to discover after the move to Indiana--4 years after the move--in 1943.

Among the memorable comments from my scientific friends in the early days of mating types, one stays vividly in my mind. It was a comment of Harry Eagle who is famous for the culture medium he devised for mammalian tissue cells. When he learned from me about mating types, he said the first thing he'd try to do would be to find out what substances on the cell surface acted as mating type specific reactants. Here it is now 41 years later and, in spite of many efforts by many people no one yet knows what the mating type substances are in any species of Paramecium. And only in the last few

years have they been identified in any Ciliates--only one of the two being known in one species of Blepharisma from the brilliant work of Miyake and his collaborators.

Now I shall tell how my career at Johns Hopkins came to an end and with my migration to Indiana. In Dec. 1938, the Amer. Assoc. Adv. Sci. met at Richmond, Va. There was to be a symposium on mating types sponsored by I think the Zoologists and Naturalists. Jennings was to chair and organize it. The other speakers were to be Kimball, Giese (who had confirmed our findings in P. multimicronucleatum), and me. After I had prepared my speech, I came down with a bad case of measles (at my age!) and leaving my bed for Richmond was out of the question. So Jennings had to introduce the symposium, give my speech, and give his own. Now it happened that Fernandus Payne went to the symposium "to look me over" as a possible candidate for ~~ENK~~ an open faculty position at Indiana. Ruth and I had met Payne at Woods Hole one summer when he and we sat at the same table at the MBL mess. Ruth had a job then and could come to Woods Hole for her vacation only. She had written to ask me who was at our mess table and I had replied that, among others, there was a nice old gentleman named Payne, from some teacher's

college in the mid-west! That must have been in 1930 or soon thereafter. Little did I suspect that this "teacher's college" was the university in which I'd spend most of my life and career. Well, when I didn't show up at Richmond, Payne later (1939) came to visit me at JHU. Meanwhile, Jennings had retired (1938) and had gone to Los Angeles. His successor as chairman of Zoology was S. O. Mast, who, I had long suspected, was anti-Semitic, though this may have been a misjudgement on my part. I always thought that he held against me the fact that I had rejected him as a Ph.D. sponsor and had turned instead to Jennings; but again I may have misjudged him. He had advised me, after my discovery of mating types, to seek a more favorable Ciliate than Paramecium for genetic work. And he had also advised me that it would be best for my career to leave JHU and make my mark in a place where I had not been a student. I took all of these things as manifestations of his anti-semitism, but they were not necessarily so, and may have been honest attempts to advise me well. No matter. The word got around that Mast was anti-semitic and that he was not treating me well. When Payne approached me about Indiana and I began to react favorably, the issue of anti-semitism

at JHU was picked up by the newspapers.

Payne invited me to come to Bloomington to give a seminar and to be looked over. I went. That year (1939) was one in which the Judas tree (Redbud) and dogwood were especially luxuriant and were out together in the first part of May. I arrived via the B & O RR at Bedford, was met by Brenemann who drove me to Bloomington. I gave a seminar and recall in the audience the rapt attention of the Krocs, especially Alice whose ~~high~~ bright eyes and beautiful face drew me to her at once. But after the seminar, the famous Molnkaus (who had been prominent in demonstrating the continuity of chromosomes) startled me with the deflating comment, "It's too beautiful, I don't believe a word of it". Like Wenrich's "I've seen it and still I don't believe it" after seeing my demonstration at Woods Hole! So, I assumed the people at Indiana would not make me an offer, but put me down as a faker. When it was time for me to go back to Baltimore, Payne said I'd hear from him within a week or ten days. Bill Ricker drove me back to Bedford, but we got there sometime before the train was due and he used the time to take me to Spring Mill Park. We went down the hill to the cave where Eigenmann had studied the blind fish and while there we heard the whistle of the train.

We ran up the hill and when we got to the train it was just about to leave. Bill threw my bag on the train and I hopped on breathless. It took a long time for me to calm my heart and breath at normal rate, but at least I was on my way home.

Ten days went by and no word from Payne. But eventually an offer came: \$4000 as Assistant Professor. I held out for Associate Professor at the same salary. Payne went back to his faculty which included two Assistant Professors who had been there some years and should not have been jumped over by a new recruit. But they handsomely agreed to have me come in over their heads. Dean Stout was annoyed at the "high" salary being offered to me and wanted it to be \$3500. But Payne held out. My salary at JHU was \$2500 and had been the same for 9 years in spite of promotions from Research Assistant to Research Associate to Associate. Word of the offer from Indiana got to the Baltimore Sun paper and reporters quizzed me on whether I was leaving and if so whether it was because of anti-semitism. I refused to answer their questions, but ~~however~~ the papers played up the idea that JHU was unable to hold its good men from outside offers, even from a little known midwest state university. The President

of JHU, Isaiah Bowman (the famous geographer who had been important in fixing national boundaries after the 1st World War), called me to his office and said some incredible things to me, something like this: We want you to stay here. We will promote you at once to Associate Professor at \$3500. After all, it should be worth something to you to be at a great university like JHU--at least worth \$500 a year. More, we will promise to promote you to Professor in due time. But in all frankness, Sonneborn, I must tell you that you will never be made Head of the Department. As a Jew, you would be subjected to irresistable pressures to take Jews in your Department and that would make the non-Jews leave. It would ruin the Department.

I consulted widely what to do. The famous Raymond Pearl at the Medical School had been Visiting Patton Lecturer at Indiana and I consulted him about I.U. He claimed credit for suggesting me to Payne and advised me to accept. I.U. had a new young President, Herman Wells, who would probably go far towards ~~improving~~ improving I.U. I also consulted with Ralph Cleland whom I had known well while he was at Goucher College in Baltimore and who had gone to I.U. a year before my call came. (He probably had more influence on Payne and Wells than Pearl had.) Finally, I went to Princeton to consult

Flexner at the Institute for Advanced Study. He too advised accepting.

He was a leader in American higher education and knew the midwest, having come from Kentucky. So, I accepted. To my Eastern Seaboard-bound friends, anything west of the Appalachians was primitive backwoods. I'm sure they thought I was crazy to leave JHU for I.U. But in spite of the marvellous opportunity for research I had had at JHU and the security it had given me during the long hard years of the depression, I was glad to leave. JHU had become greatly weakened during the depression. Many of its top people, such as the Nobilist Physicist James Frank had left (he went to Univ. Chicago) and whenever I met him thereafter he suggested that we should found a club of stars who left Hopkins! Worst of all Hopkins was smugly living in the past--on its past reputation--not looking ahead; while I felt a stirring at I.U., a determination to be great in spite of little past glory to exult in. That appealed to me. I wanted to be part of the fulfillment of that ambition and, as a teacher, to try to inspire a generation of midwest students. So I came to Bloomington full of hope, expectation, determination and excitement. Full of energy and drive, Ruth and I and our two boys drove to our new home in August 1937. I had told her we'd probably stay there only

a few years and then migrate again. For Payne had admitted to me that he didn't expect to be able to keep me long, but that he felt it would be good for I.U. if I stayed only a year or two. So, what actually did happen at I.U. to hold me nearly 40 years will have to be accounted for in what follows.

Before I put Baltimore behind me in this narrative, I shall try to recall some of the memorable aspects of my non-professional life there. To begin with, my closest family circle. My brother David, 7½ years older than I was, was too much older for us to be sociable with each other in any way. For reasons I cannot recall, I held him in great admiration and affection. I recall vividly one event that illustrates this. He and his friends were playing ball out in the street by our house on McCullough St. (1715?) which was against the law and policemen came to clear them from the street. I remember the terror I experienced from the fear they would take David to the police station and put him in jail, which of course they did not do. Because of my adoration of David, who was headed for engineering, I wished to emulate him and so chose to go to the engineering preparatory high school, the Baltimore Polytechnic Institute. The transition to it from primary school seemed to me to be a major new adventure in my life; I supposed that it would be very different from anything I had experienced and I wanted to be prepared for it. So I asked David to brief me about it. He reacted in a way that hurt me, saying in effect "No one briefed me, so why should I brief you?" And he didn't.

During my first two years there, I was first in my class; but, in the course of those two years, I metamorphosed into a person in my own right, discovered that my interests were humanistic, not engineering, and transferred from B.P.I. to the Baltimore City College (a high school in spite of its name!), where the emphasis was on a liberal education, for my last ~~XXXX~~ two high school years. My years at High School, especially the early years, were traumatic. I was going through a period of philosophical and religious questioning and had rather intense "discussions" with my parents (which too often took the form of a monologue passionately delivered by me and painfully heard by my mother, sympathetically by my father). My brother never went through such a period and had no comprehension of what was going on in me. He apparently thought it was funny and that I was simply dramatizing to call attention to myself. Again he hurt me deeply by commenting "end of Act. 1. Curtain!" when I paused a moment in the midst of one of my deeply felt "monologues".

Eventually, it became apparent to me that my brother was more interested in money than in contributing constructively to society. After a few years as a practising electrical engineer, he gave it up completely and went into

the furniture business, at which he became successful. This was a blow to me,--he had broken faith with what I had believed were his ideals.

From that time on, I privately ceased to admire him and became suspicious of his motives and actions, but publicly the ties of blood were never broken. Even his attempts to assume the responsibility of older brother-- as when he once tried to warn me against the evils of the sexual mores of some young men--amused rather than instructed me; but I kept my amusement to myself.

I must add that my brother was left with family responsibilities when I moved to Bloomington. He took mother and father into his home-- a mansion in the suburbs designed to have a semi-private wing for them. My father had a decade or more of suffering from high blood pressure, angina pectoris, and heart block through all of which he kept his sunny disposition and the urge to use his energies as much as his debilitated condition would allow. He was a warm person and, I think, a joy to all around him, including my brother's wife, Freda. But my mother was a worrisome person--she had reason to worry about my father's health--and worried about her own health, too. My father died Jan. 3, 1944, at 71, only a few

weeks after I spent a week with him in the midst of a lecture tour on the east coast. During that week, he and I had wonderful days and evenings together and I delighted to be able again to tell him how much I appreciated what he had meant to me in the years of my adolescent revolution and ever since. In turn, he tried to anticipate the needs I would or might have in the future in providing for my family. So, he carefully explained to me how to take care of savings and how to invest them, because he correctly knew that money meant nothing to me so long as I had enough to live on very modestly and that I was untutored in the extreme about monetary matters.

My mother, 4 years older than my father, died in November of the same year. My brother and I were executors of their modest estates, but I being away and my brother being the business man of the family, I left matters pretty much in his hands. This required much correspondence between us and, I fear, brought out the worst in him. In effect, he wrote me things that could have--but did not--poison my mind about my mother,--in regard to her relations to and feelings about his wife and mine. Over this, the split between my brother and me surfaced and we remained very cool to each other for years. Only when he became quite ill and his wife told me how much

he wanted to heal the old wounds before he died did I make an effort to do so. We visited them at Sarasota as often as we could and our relations were good during the last 6-10 years of his life. He died a few months before his 76th birthday in 1974.

Among my many other relatives who lived in Baltimore, two cousins were of some importance in my life. One was my cousin Lavina Bamberger, an elder spinster who was sister to Louis Bamberger and his sister Mrs. Fuld who founded the Institute for Advanced Study at Princeton. Louis had made a great fortune in the department store business, L. Bamberger and Co. of Newark, N.J. He considered ~~going~~ giving his fortune to the Johns Hopkins University on condition that it would abandon its undergraduate school and become strictly a university for graduate work. When JHU refused to accept his conditions, he founded the Institute at Princeton, got Flexner to head it, and soon saw it rise to the top by attracting to its faculty Einstein and other top scholars. I was very proud of my relationship to Louis Bamberger although I met him only once and found him a modest, gentle, unassuming man--totally unlike the commonly accepted conception of a successful business man.

His sister Lavinia became interested in me when I was a child and early nourished the interest I was developing in art. I drew a great deal. She gave me albums of reproductions of great paintings and drawings. When somewhat later I became entranced with music, she took me to symphony concerts. She was a great lady with an active mind of her own and I liked her very much. After I married Ruth, she took us in as companions on her excursions to her summer home on the Magothy River south of Baltimore, for she and Ruth got along famously. She was a sponsor of social work activities and Ruth was a social worker, an initial bond between them, which was strengthened by their obvious delight in each other's company.

The other cousin who impinged significantly on my intellectual life was ~~MYXKXMX~~ Nathaniel Hirsch, son of my mother's sister, who lived in Nashville, Tennessee. A bit older than my brother, Nat early recognized my intellectual interests and on his rare visits to our family took the opportunity to say mysterious things to me and to recommend books. He got his Ph.D. at Harvard under the distinguished Briton, William MacDougall. Nat was however a mystic and symbolist under the influence of his older half-brother Sidney who was the all-around most learned man I ever met.

Nat had another half-brother who was in an insane asylum, ~~but whom Nat and Sidney considered~~ but whom Nat and Sidney considered to be the incarnation of the holy spirit. They believed in the reincarnation of trinitities of great minds (Socrates, Plato and Aristotle being one of them and they being another. Nat being the Aristotle of this reincarnation.) All of this came out to me only gradually over the course of the years.

I recall one visit Ruth and I had from Sidney in which we talked through most of one night. Among other things, he had me bring out the Bible, turn to the 48th psalm, count 48 words from the beginning and 48 words from the end and put the two words at these positions,--shake and spear--together. This Shakespeare was to him proof that Shakespeare knew human beings had 48 chromosomes! When, years later, it was discovered that human beings have only 46 chromosomes and I called Sidney's attention to that, he --and the whole "trinity"--were deeply disturbed. They remained disturbed and believed that 46 must be an error. If they had only known that rarely there is a man with two extra Y chromosomes--total 48--they would have been comforted and settled for 48 being characteristic of "superman"! But Sidney did not live long enough to learn about this. Ruth was always worried that these